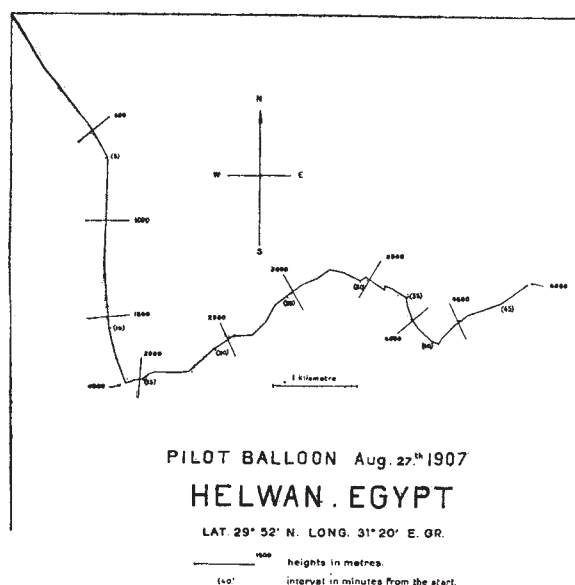


## LETTERS TO THE EDITOR.

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## Upper Air Research in Egypt.

THE Helwan Observatory (under the Survey Department of Egypt) has recently acquired apparatus to enable it to join in the study of the upper regions of the atmosphere. As a commencement, some fifteen ascents of small "pilot" balloons were made during the month of August. On three occasions the balloons were watched to a height of 5000 metres, but rather a large percentage of the balloons used burst at much lower altitudes. A better type of balloon has been ordered, and it is hoped that observations may be regularly made with them up to 5000 metres or 6000 metres. The balloons are of 2 feet nominal diameter, and are filled with dry hydrogen made from zinc and sulphuric acid. They are observed as they ascend by two observers at the ends of a base line 1 kilometre in length. The theodolites are of the very convenient type made by S. and A. Bosch, of Strassburg, specially for this work. The result of a month's work shows that at this season the surface wind (N. to N.W. as a rule) is from



1000 metres to 2000 metres thick. Above this there is a layer of varying thickness of winds from W. to W.S.W., whilst above 4000 metres other winds are reached, but the number of observations is at present too few to generalise about this region. An interesting ascent is shown in the figure, which represents the horizontal projection of the flight of a balloon on August 27, with contours showing the position of the balloon at intervals of 500 metres. In this case, above the stratum of S.W. wind there was a layer of N.W. winds, whilst above this a S.W. current was again entered.

Besides this study of the winds, kite ascents will very shortly be commenced. The apparatus acquired for these consists of a winch of the pattern designed by Mr. W. H. Dines, F.R.S., driven by a Crossley petrol engine of 4 horse-power. The observatory is indebted to Mr. Dines and also to Mr. J. E. Petavel, F.R.S., who watched the manufacture of the winch and introduced many minor alterations which use of a similar machine at Glossop had suggested. The machines are housed in an iron building on the flat desert plateau behind the observatory.

During the September international days (September 4, 5, and 6) five ascents of pilot balloons were made. The height to which they were followed varied between 2500 metres and 3300 metres.

Helwan.

B. F. E. KEELING.

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## Newton's Rings in Polarised Light.

AN erroneous statement regarding the above-mentioned subject is made in Preston's "Theory of Light" (p. 363, 1901 edition) and also in Edser's "Light" (p. 519, 1902 edition). As the error is a rather serious one, it seems worth while to point it out.

When the rings are seen between two lenses of the same substance, by light polarised perpendicularly to the plane of incidence, reflected at an angle greater than the polarising angle of the substance, it is stated that the centre of the rings is bright. That this is wrong can be seen. For:—

(1) Stokes has shown from the principle of reversibility that, whatever be the nature of light, the centre of the rings seen between lenses of identical refractive indices is black at all incidences of the light.

(2) Since the centre of the rings is black at all incidences for common light and for light polarised in the plane of incidence, it follows by resolution that it is also black when the light is polarised in a perpendicular plane.

(3) When the angle of incidence is less than the polarising angle, the coefficients of reflection in glass and in air at the bounding surfaces of the two media are opposite in sign. It is argued that, on increasing the incidence, the coefficient of reflection in air changes sign as the polarising angle is passed, and therefore at such incidences the two coefficients agree in sign, and destructive interference no longer takes place. Really, however, it appears from Fresnel's formula (coefficient =  $-\tan(i-r)/\tan(i+r)$ ) that both the coefficients change sign as the incidence passes through the polarising angle, and therefore continue to differ in sign, as can be directly shown from the principle of reversibility. Destructive interference does, therefore, take place.

(4) I have shown by experiment that the statement is not true.

(5) Airy has shown (Lloyd's "Wave Theory," p. 178, and Jamin's "Optique Physique," p. 503) that when the two lenses differ in refractive index, the centre of the rings seen in light polarised perpendicularly to the plane of incidence is white only when the incidence lies between the angles of polarisation of the two media. Outside these limits the centre is dark.

C. V. RAMAN.

Science Association Laboratory, Calcutta,  
September 12.

MR. RAMAN'S criticism of the statement made on p. 519 of my "Light for Students" is quite justified. Some time ago I noticed the error myself, and devised the following experiment, to which the same objections cannot be raised, while at the same time it is more easily performed than that in which two lenses of different refractive indices are used.

An ordinary black tea-tray is filled with tap water, and the surface is then touched by the end of a glass rod which has been dipped in oil (I find that the heavy paraffin oil used for engine lubrication answers well). The oil spreads over a fairly large area, Newton's rings being exhibited round the edge of this. On viewing the colours through a Nicol at an angle slightly greater than  $45^\circ$  no change is produced when the light transmitted is polarised in the plane of incidence, but on turning the Nicol through a right angle the colours change to their complementaries. In this case the light is reflected from the lower surface of the film at an angle slightly greater than the angle of polarisation for that surface, while it is reflected from the upper surface of the film at an angle less than the polarising angle. I presume that if Lloyd's single mirror fringes were observed through a Nicol, a similar change would occur on rotating the Nicol; I should be obliged if anyone who has tried this experiment would let me know whether this actually occurs. EDWIN EDSER.

## Thermodynamics of Diffusion.

IN his review of "Thermodynamics" (NATURE, July 25) and again in the *Philosophical Magazine* for July, Mr. Burbury directs attention to a result stated by me regarding the gain of entropy resulting from slow diffusion of gases at constant pressure and temperature.

May I direct attention to the context in connection with

which this result is stated in "Thermodynamics" (§§ 124, 126, 156)?

It cannot be deduced from the laws of thermodynamics or the definitions of a perfect gas (§ 124). These leave the change of entropy in the form of an undetermined constant.

It must necessarily be based entirely on experimental evidence (§§ 126, 156). It is in all probability *approximately* true for actual gases, but of this the experimental physicist is the only competent judge. As applied to "perfect gases" it should be regarded, in common with Boyle's law, as one of the "definitions of a perfect gas," a definition selected partly on account of its simplicity and partly on account of its approximate agreement with the properties of actual gases (§ 156).

An irreversible transformation does not, *ipso facto*, imply a gain of entropy. Unless a compensating transformation exists (§§ 50, 51), and unless the final result involves nothing more than a loss of available energy, we have no justification for applying the methods of thermodynamic analysis. If diffused gases could never be separated, we should have an instance in point; but do such exceptions exist?

Mr. Burbury asks why should different gases behave differently from different portions of the same gas? This question must be decided by the experimental physicist, subject to some further condition, *e.g.* that the gases are in the presence of a liquid which dissolves one of them or of a membrane which is permeable to one of them only. In other words, the matter resolves itself into the question, Why should the conditions of equilibrium of a gas in such circumstances depend on its *partial* pressure instead of on the total pressure of the mixture?

If the experimental physicist had told me that the total pressure, and not the partial pressure, was the determining factor, I should have asserted that no entropy was gained by diffusion, and should have written zero as the value of my constant C.

But then we should have no vapour of water in our atmosphere unless the temperature rose above the boiling point of water. These, generally speaking, are the views which the book was intended to convey; but may I direct attention to the large number of open questions in thermodynamics that have hitherto only received scanty attention in the hands of mathematical physicists?

G. H. BRYAN.

In the passage in my review to which Prof. Bryan takes exception I had in my mind his definition of available energy at p. 35 and p. 43:—"The available energy of a system *under given conditions* is the quantity of energy which *under these conditions* can be converted into work"; and in the same passage the conditions are also spoken of as "external" conditions. Let the system consist of two gases occupying equal halves of a cylinder, both at the same temperature and at pressure *p*, separated by a piston impervious to either, and the whole surrounded by air at the same pressure *p*. It seems to me to be impossible under those conditions to convert any of the energy of the system into work; but if it can be done, it must be possible to explain how. The context of p. 125 does not seem to me to explain it.

S. H. BURBURY.

#### The Nomenclature of Radio-activity.

THE name "ionium" which Dr. Boltwood proposes for the new radio-active element, of which he announces the discovery in NATURE of October 10, is open to serious objections. I do not mean merely linguistic objections—it is too late to consider them; beside such a hybrid as "ionisation" the philological barbarity suggested by Dr. Boltwood is insignificant; but it is a first principle of scientific nomenclature that a name should connote some of the distinctive properties of the thing named. A thoroughly satisfactory system for naming radio-active elements has not been put forward, but that adopted by Prof. Rutherford in designating the members of the series descended from radium is at least better than none.

According to this system, the products arising successively from the disintegration of a radio-active element are denoted by the name of that element followed

by the letters X, A, B, C, &c. The principle of this plan has been adopted by universal consent in the nomenclature of the products of radium, thorium, and actinium, but for historical reasons slight divergences from the simplest form of the system have been permitted. Only one disintegration product of uranium (other than the radium series) has been known hitherto; its name, uranium X, is in accordance with Prof. Rutherford's nomenclature. Dr. Boltwood now announces the discovery of a descendant of uranium subsequent to uranium X; it appears to me desirable that this product should be known as uranium A, and should not be given any purely fanciful and meaningless name such as its discoverer suggests.

NORMAN R. CAMPBELL.

Trinity College, Cambridge, October 12.

#### On Correlation and the Methods of Modern Statistics.

IN my last letter (October 3, p. 566) I ventured to express the modest hope that "an astronomer may be permitted to dissent from these applications of modern statistical methods." Prof. Pearson refuses the desired permission with such warmth of language and wealth of argument that I find it difficult to make a suitable renewal of the request. Perhaps I may be allowed to confine my reply to the point of most general interest.

With regard to the supposed relation between magnitude and colour, Prof. Pearson wishes "to say a strong word" about my criticism of a conclusion respecting the bulk of the lucid stars, which I said was based on a record in which the white stars had no frequency. I have re-read Miss Gibson's paper, and am unable to see that my criticism in any way misrepresents the facts.

In section (3) of her paper Miss Gibson discusses the relation between magnitude and colour, basing her results upon the Cape list of 159 stars, from which all stars less coloured than deep yellow are excluded.

In section (4) she fits various types of frequency curve to statistics of counts of the lucid stars. In this part of the paper there is no mention whatever of colour or spectral type.

At the end of section (4) we have the conclusion, to which I ventured to take exception:—"Thus we have the suggestion, even if it be only of the vaguest kind, that the bulk of the lucid stars may belong to a separate universe within which magnitude is not mainly determined by parallax or distance, but is more closely associated with colour, and thus probably with chemical or physical condition." The phrase "*but is more closely associated with colour*" is undoubtedly there. If it does not arise from section (3), its origin is "wrop up in mystery"; if it does arise from section (3) my criticism was not so unjustifiable as Prof. Pearson would with strong words call upon the reader to believe.

ARTHUR R. HINKS.

Cambridge Observatory, October 18.

#### New Zealand Birds.

DURING the past twenty-five or thirty years many reports have been published in regard to the extinction of New Zealand birds, and an impression has gone abroad that our avifauna, with its striking peculiarities and its wealth of interest to ornithologists, will soon be lost. Some time ago, when I was inquiring into the results of the acclimatisation of English birds, I had thousands of circulars distributed in all parts of the colony, and on those circulars I placed questions dealing with the position of the native birds. When the circulars were returned to me I found that every native bird was accounted for, in some cases in many different districts.

I feel, therefore, that I am able to sound a brighter note than has been sounded by most writers on New Zealand ornithology. From personal observations, I can say that several species the extinction of which was announced twenty years ago are fairly plentiful, and are increasing. I may mention specially the stitch-bird (*Pogonornis cincta*), the bell-bird (*Anthornis melanura*), the North Island robin (*Miro australis*), and the tui (*Prothemadera novae zealandiae*).

I do not know of a single New Zealand bird which we can say with any degree of certainty has become extinct since European occupation of the country, except perhaps